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CONVECTION AND STRESS PROPAGATION IN THE UPPER MANTLE

by

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June 15, 1967

Technical Report No. 5, Prepared under the Grant NsG-556 to Princeton University by National Aeronautics and Space Administration

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CONTENTS

1. Introduction and Summary

Continental topography and land-based geology have failed to exhibit quantitative relationships of large dimensions. In more recent times a number of such correlations have been brought to light by ocean-bottom topography and geology. Here belong the characteristic locations of the submarine ridges with respect to continental margins, the fairly uniform depths of the abyssal plains, and the indications that the ocean floor has moved and is still moving in a direction away from the active submarine ridges. These correlations present evidence for definite dynamical processes in the upper mantle; the latter are now often interpreted as due in the main to thermal convection currents.

The upper mantle is here taken as relatively shallow, of about 800 km depth. Following F. Birch we assume that the lower mantle has been drained of its radioactivity and possibly of much of its lighter constituents in a comparatively early phase of the earth's history. This was accompanied by general convection in the lower mantle, leaving the latter with a low (adiabatic) temperature gradient and no heating, so that it is now convectively inert.

The most striking feature of upper-mantle convection is the preponderance of horizontal motions. This can only in part be attributed to the relative shallowness of the layer; it is mainly caused by a rather pronounced minimum of the "viscosity" between about 100 and 200 km depth. The existence of such an "asthenosphere" is readily explained by the fact that the closest approach to melting must occur in this region. It may be shown by mechanical arguments that horizontal sliding of the top layer, here called "tectosphere", can be more easily achieved than circulation in the material underneath.

Mechanical stresses are preferentially transmitted horizontally, along the tectosphere. This accounts for the otherwise quite unintelligible correlation between ridges, considered as upwellings in the mantle, and continental margins which are obviously rather shallow features on a mantle-wide scale.
We conclude that the convective circulation in the upper mantle is largely controlled by irregularities of its free upper surface, in a manner inverse to the way in which thermal convection of a fluid is often controlled by the corrugations of its lower boundary.

In the past, mantle convection was often dealt with by models drawn from fluid mechanics. But there are great differences between solids in creep and fluids. While not using calculations from the difficult theory of non-linear creep, I have tried to be extremely circumspect in applying fluid-mechanical concepts to the mantle. In this I had the counsel of my colleague E. Orowan whose knowledge of the mechanics of solids proved repeatedly invaluable.

2. Thermal Stratification of the Mantle

The key to all models of convection in the earth's mantle is the distribution of temperature with depth. Unfortunately, we have no quantitative knowledge of the temperature that would be even remotely comparable to our precise knowledge of many mechanical properties as obtained from seismic data, and we must be satisfied with semi-quantitative estimates. If the temperature gradient as measured near the surface (some 20°-30°/km) would continue downward, the mantle would be molten at a depth of less than 100 km. In order to avoid this conclusion the temperature gradient must be assumed to decrease rapidly with increasing depth: the temperature-depth curve must flatten out, that is, it must have a relatively pronounced bend at rather moderate depth (see Fig. 1, below). Even a qualitative study of the possible causes of this bend turns out to be instructive. We find that there are concurrently three such causes:

1) In the first place, there is the gradual transition with rising temperature from ordinary conductive heat transport to radiative transport, which at higher temperatures is more efficient than lattice conduction; at sufficiently high temperatures, heat is carried almost entirely by radiation. Depending on the infrared transparency of the material, this change of mechanism occurs for silicates in general in the neighborhood of 1600°-2200° K. Calculations of heat distribution in the earth's interior which

have been based on this phenomenon (e.g., Lubimova, 1958; MacDonald, 1959) show very clearly the appearance of a rather sharp bend in the temperature curve owing to this effect alone.

2) Next, there is the concentration of the radioactive heat sources. Birch (e.g., 1965) has long since advocated the view, also propounded by H. Jeffreys, that the radioelements, owing to their large ionic volume, have tended to rise in the mantle and are now very strongly concentrated in the upper mantle. According to Birch this process of segregation should have taken place in a time of the order of a billion years from the beginning stage of the earth (i.e., the stage in which the core was formed). Patterson and Tatsumoto (1964) have interpreted their studies on the distribution of lead isotopes in an entirely similar fashion; they say furthermore that such a model is about the only one compatible with their experimental results. Another, independent argument pointing in the same direction comes from the estimates of the central temperature of the earth that have been based on the existence of a solid inner core. A number of authors agree that this fixes the temperature at the bottom of the mantle as not much in excess of, say 4000° K. Since the mantle is nearly 3000 km thick and the temperature must certainly rise to 1500° K rather close to the surface the <u>average</u> temperature gradient over all but the topmost layers of the mantle should be below 1°/km; the gradient in the lowest 2000 km, say, should be

appreciably less than 1°. Again, by a totally different approach, on the basis of electrical conductivity data, Tozer (1959) has estimated the temperature of the lower mantle as of the order of 4500°, in fair agreement with the preceding value considering the crudeness of the arguments. Such relatively low temperatures and low gradients are hard to reconcile with the idea that the lower mantle has retained most of its original heat, as well as its radioactivity, and at the same time has never been convecting. If there was (solid-state) convection in the past, the lower mantle is also more likely than not to have undergone a degree of chemical segregation which would certainly include appreciable upward migration of radioelements--a point to which we shall revert presently.

Furthermore, if one wanted to justify as low a temperature gradient as 1°/km in terms of radiative transport alone, the mantle would have to be highly transparent in the infrared (since at these temperatures the black-body curve reaches its maximum in the near infrared). Now it is a known fact that the prime absorber in the magnesium-iron silicates of the mantle is the Fe ion. The mantle will be transparent and a good radiative conductor only if it contains very little iron. On the basis of seismological data, D. L. Anderson (1967) has estimated that the atomic Fe/Mg ratio in the mantle is roughly as follows: Above 400 km depth 1:4, below this depth 1:3, and a somewhat increasing iron fraction at still greater depth. With that much iron, it would be difficult to maintain a very low temperature gradient by radiation alone if there are strong heat sources at depth.

3) The most efficient way of reducing the temperature gradient is convection itself. Since convective transport is much more rapid than any transport by radiation or ordinary heat conduction, the temperature during vertical displacement changes along the adiabat. Using a thermodynamical identity, the adiabatic temperature slope as function of depth can be written as

$$(\partial T/\partial z)_S = g\alpha T/c_p \tag{1}$$

where α is the coefficient of thermal expansion at constant pressure and the other symbols have their usual meaning. Numerical estimates for the upper mantle lead to values in the range of 0.25-0.40°/km. Thus in the presence of very active convection only small vertical temperature gradients can be expected. (This statement, however, will have to be greatly modified for convective patterns that are very shallow compared to their width, such as found in the upper mantle; see Elsasser, 1966.)

Later on we shall discuss reasons why convection in the mantle must be considered from the viewpoint of solid-state <u>creep</u> which is a highly non-linear phenomenon, and why a model in terms of a fluid with conventional linear viscosity might not be adequate. We shall use the term <u>ductility</u> to indicate qualitatively the ease with which a solid in creep can be deformed by a given stress. One thing which will be discussed more quantitatively later on is clear at the outset: ductility increases strongly with increasing temperature.

Now it seems rather senseless to discuss pronounced ductility of the mantle due to heating without also considering true melting. The two phenomena could only be separated in a strictly homogeneous material which remains crystalline and hard throughout, all the way to the melting point, and then turns liquid at a sharply defined temperature. But the mantle is quite heterogeneous chemically; some lesser components have lower melting points than its bulk material. As Clark (1963) has pointed out, the melting point may be further lowered if these low-melting components form eutectics. We should note here how inhomogeneous the mantle probably is, since this is not always sufficiently emphasized in the geophysical literature. One can get a good idea from the measured values in meteoritic matter where the atomic abundance ratio of the four most abundant lighter ions to the magnesium ion is (Mason, 1958)

$$(Al + Ca + Na + K) / Mg = 0.21$$

If we multiply the Mg abundance value by 1.25 in order to account for the presence of iron in the upper mantle, we still find that, roughly, the ligher silicates should amount to over 15% (say, by volume). We might therefore readily encounter situations where the mechanical behavior of the mantle with respect to slow deformation such as creep is almost completely determined by its lighter components, while at the same time these light components may have only a modest influence upon the seismic properties.

A term such as "melting temperature of the mantle" is quite ambiguous but the following general conclusions seem justified. From melting-point curves as function of pressure determined in the laboratory one infers a rather regular rise of melting point with depth. For possible constituents of the upper mantle, the melting-point gradients as determined from laboratory data range from 1.4°/km to about 3°/km (Clark, ed., 1966). Since these gradients are considerably steeper than the adiabat, equation (1), there follows the well-known (Adams-Williamson) result that a molten mantle will solidify from the bottom up. On using a model of a homogeneous mantle without creep the temperature curve in the lower part of the mantle should run close to the melting-point curve of the major silicates. (Such an argument is not affected by the idea that the earth aggregated in a cold state and became molten only later on by the combination of two effects: first the temperature rise due to the formation of the core, or by completion of the core formation if the core was partially formed already in the gas-dust stage;--secondly by the comparatively high heat output of the radioelements at this early stage.) But the temperature gradient is radically altered when solid-state convection is present. In fact, if the mantle contains low-melting components and if, moreover, some components remain highly ductile even after solidification, solid-state convection can be expected to set in long before melting of a thick layer has occurred. Such convection will undoubtedly be associated with a partial upward segregation of the lighter constituents; the upward migration will be most pronounced for those ions which have the largest radii (even if they are not light); among them are of course the radioactive elements.

In this way we can give an answer to Urey's famous argument that the earth can never have been molten, since an earth once molten would thereafter be tectonically "dead" because the light constituents and the radioelements would all be at the surface. In place of this we may now assume partial segregation in a mainly solid but convecting mantle. Such segregation may long since have reached a terminal stage in the lower mantle while still going on actively in the upper mantle. If the lower mantle is assumed to be some 2000 km thick it containsnearly half of the entire volume of the mantle; hence the migration of any constituents into the upper mantle will not change relative concentrations there by more than a factor of about two, usually by less.

Next, we should remember that the temperature distribution in a convecting medium tends toward the adiabat. The adiabatic gradient is considerably smaller than the melting-point gradient. Thus if the lower mantle was convecting in an earlier stage of the earth's life, it should have assumed a temperature distribution close to the adiabat. If this is combined with an efficient removal of radioactive heat sources from depth, it would lead to a condition where by now the lower mantle, if it has any convection at all, moves so slowly as to be effectively uncoupled from the upper mantle.

It is only realistic to think of the ductility of any layer of the mantle as time-dependent on a geological scale. Such changes are especially to be expected after the lower mantle has ceased to convect and may have lost a fraction of its lighter constituents. It might then change its ductility gradually, becoming less ductile as time goes on. This occurs through recrystallization processes, thermally activated movements of dislocations, etc. Such phenomena are well known from ordinary life in the form of increasing brittleness of material with passing time. Of course brittleness is absent in the lower mantle but ductility can be expected to change in just the same way. Thus efforts at explaining the disequilibrium equatorial bulge of the earth (amounting to about 60 m height) by pronounced "hardness" of the lower mantle (MacDonald, 1963; McKenzie, 1966) might point in the right direction. However, the assumption of a linear viscosity at low stresses must be questioned; if it does not hold, the numerical "viscosities" of 10^{26} obtained may be irrelevant.

3. Thermochemistry of Convection

It will be convenient to divide the following analysis of thermal and chemical aspects of convection into four parts: A) Cyclic and acyclic convection; B) Phase transformations; C) Thermal factors in creep; D) Chemical factors in creep.

A) Cyclic and Acyclic Convection. There is no traditional, generally accepted definition of convection. The best thing we can say is that convection is motion which results from density differences in a gravitational field. Such density differences can be either thermal or chemical, and all intermediate cases are possible when complex phase transformations occur. Now these changes can be either *cyclic*, implying that the material runs through a thermodynamical cycle as it rises and then sinks to rise again eventually; or else they can be *acyclic* as exemplified by the selective rising of a lighter fraction which leaves a heavier residue behind. This may lead us to ask whether the slow falling of a heavy piece of matter, or else the rising of a light one, should be considered as a convective process. Our answer will be that verbal scholarship and science do not mix too well; after having recognized the ambiguities of a term we keep on using it as best we can.--In dealing with the motions of the upper mantle we are clearly facing a highly complex situation: A cyclic phase transformation that involves expansion of some component on rising, and contraction on sinking, may become an acyclic process through the physical separation of the less dense component from the denser one. One cannot doubt that convective processes in the mantle are in this sense always partly cyclic, partly acyclic. We shall leave the degree of each of these open; this remains a matter of specific models and of specific geological situations which may vary both in space and in time.

While the acyclic component of convection is extremely important from the geological viewpoint, its contribution to the energy of the convective process seems to be relatively small. To show this, let us assume that during a given geological interval, say one billion years, 0.5% of upper mantle material comes to the surface irreversibly. With an overall depth of, say 800 km, a mean vertical travel of 400 km, and a density defect, $\Delta\rho = 0.5$ of the light material, we find the amount of gravitational energy released to be $4 \cdot 10^8$ cal per cm^2 of surface. This is to be compared to an aggregated heat flow of some $4 \cdot 10^{10}$ cal/cm^2 which has left the earth during the last billion years of geological history. (That is, if the present heat flow were constant; actually the heat flow has been somewhat larger in the past.) Not all of this heat is available to generate convective motion but only a fraction, $\Delta T/T$, by well-known thermodynamical principles. It would seem, nevertheless, that the larger portion of the energy sources driving convective motions in the upper mantle is of purely thermal origin. Details will depend critically on the efficiency of the convective engine.

Note on the Core. The following is a brief digression into the thermodynamical nature of convection in the core. We know that such convection is required to account for the generation of the earth's magnetic field. Now and then efforts are resuscitated to account for this field by various other means, especially by generation of core motion through tides, or through equinoxial precession. However, the equations of magnetohydrodynamics say that magnetic fields destroy or create vorticity, as the case may be, and at a rapid rate if the field is sufficiently large (as observations show it to be). Hence a magnetohydrodynamic system requires a primary motion-generating mechanism that supplies more than small amounts of vorticity. No such mechanism other than convection is known.

Cyclic convection in the core should result from its overall cooling (there being very probably no radioelements in the core on chemical grounds). Heat transport in the adjacent mantle is essentially radiative; on the other hand radiative transport in the core should be negligible because metals are highly opaque. Conventional heat conduction in the core should be of moderate magnitude but not large; this is seen as follows: The thermal and electrical conductivities are proportional to each other by the law of Dulong-Petit, but the core is not a very good conductor; its electrical conductivity is by a factor of at least 10^3 smaller than that of the well-conducting metals. Whether in these circumstances heat transport in the lower mantle exceeds that in the core sufficiently so that purely thermal convection of the core results, is difficult to decide at present.

Acyclic convection should be expected in the core: Starting from the fact that the mechanical motions are dominated by the Coriolis force one can estimate the variations in density required to balance this force (Elsasser, 1955). These turn out exceedingly small, of order $\Delta\rho/\rho = 10^{-9}$. Now there is no reason to assume that either the upper or the lower boundary of the liquid core should be in thermodynamical equilibrium. Studies of recent years (e.g., Knopoff and MacDonald, 1958) have made it almost certain that the impurities dissolved in the outer core are of the general magnitude of 10%. From the dynamics of motions in the core one may infer that the turbulent mixing time will be of the order of 500 years or slightly larger; thus at most 10^7 "mixings" of the core have taken place in the lifetime of the earth. With the above value of instantaneous density variations this would give a total density change of the core over the age of the earth of order 1%.

Looking at this problem from the viewpoint of specific mechanisms, it seems clear that atoms or ions can enter the fluid downward through the upper boundary and other atoms or ions can leave it by the reverse route. Similarly, if the inner core grows through overall cooling, there will be chemical disequilibria at the lower boundary which should contribute to convection. Such arguments show that convection in the fluid core which is demanded by the existence of the magnetic field, must also be considered altogether possible on the basis of the quite unrelated physico-chemical evidence just given.

B) Phase Transformations. A number of phase transformations have been attributed to the material of the mantle. They have been discussed for instance by Ringwood (1966). Of these, the breakdown of the main magnesium-iron silicates into corresponding oxides under pressure is the most spectacular. It is generally admitted to occur in the Birch transition zone around 800 km depth. We shall not deal with it since for reasons given at various other places of the present paper we focus our attention here on the upper layers of the mantle, above this depth. In these layers, seismological analysis reveals some further stratification, probably attributable to phase changes. With exception of the M discontinuity which separates crust from mantle these seem to be of relatively minor importance. Verhoogen (1965) has shown from thermodynamical arguments that a stream of convective motion in a medium of sufficient homogeneity of composition can cross a phase boundary; the material undergoes the phase transformation reversibly while it moves across the boundary. In this process, one may assume that a certain hysteresis occurs and that the phase boundary gets slightly deformed in the direction of the moving current. Nevertheless, Verhoogen's result indicates that models of upper-mantle convection might not depend critically on a knowledge of all the possible phase transformations; it may be possible to construct such models along lines of more direct evidence as we shall do later on.

P. Bridgman commented early in his career on the multitude of pressure-induced phase transformations that he found. There is, however, a limit to phase transformations; as the pressure increases only the simplest configurations remain stable, and phase transformations become rare again. We may surmise that phase transformations are most common in the pressure field of kilobars to some tens of kilobars found in the upper layers of the mantle. Silicates are noted for the great variety of configurations which their space lattices can assume, but many of these arrangements are rather loose and can therefore only be realized at moderate pressures. We must therefore look toward the upper mantle as the prime locus of those transformations whose cyclic occurrence (leading to expansion on rising, contraction on sinking) will be the main driving force of convection. We need hardly say that phase transformations may be considered as an extreme form of more ordinary thermal expansion. Since the density changes under phase transformations are usually much larger than those due to thermal expansion, phase changes are very efficient in generating convection.

Another reason why the upper layers of the mantle are the region where phase changes as well as convection are most likely to occur, may be recognized from Fig. 1: Near the top of the mantle the temperature gradient is far steeper than farther down. In the case of simple thermal expansion (where we have approximately $d^2\rho/dT^2 = 0$) the excess of the temperature gradient over the adiabatic gradient is a direct measure of the potential energy available in the stratification; this is the energy capable of release on convective overturn. An analysis of the thermo-mechanical principles that apply in the case of convection engendered by phase transformations is still lacking. Verhoogen's work deals with one aspect of it. Laboratory experiments with viscous fluids undergoing phase transformations would be of great interest to geophysics.

Consider now the submarine ridges as upwellings. If convection is confined to the top-most few hundred kilometers, the roots of the ridges cannot be explained by thermal expansion alone. (This would lead to very deep roots, well over 1000 km.) Instead, much of the buoyancy of these roots must come from phase transformations. Menard (1965) in his review mentions two principal types of such transformations. The first of these has been proposed by Hess (1955) and consists of the hydration of ultrabasic mantle material, peridotite, to form serpentine. According to Hess, serpentine becomes unstable at about 500° C and above this temperature dissociates into peridotite and water. Owing to this low dissociation temperature, serpentine can only be formed at rather shallow depths; nevertheless it might well make a contribution to the buoyancy of the ridges. Isotopic determinations indicate that the water is not virginal, it is presumably provided by the ocean in the vicinity.

The second major phase transformation of interest is that of basalt from its lighter, gabbroic form into the denser eclogitic form. This was first suggested in connection with the mechanics of mantle movements by Dietz (1961). In the meantime much laboratory work has been done on the gabbro-eclogite transformation by Ringwood and Green (1966) who assign to it a major role in convection of the upper mantle. Menard, however, is very skeptical about the importance of both of these types of transformations for convection. The present writer is not enough of a chemist to feel entitled to an opinion of any weight.

<u>C) Thermal Factors in Creep</u>. Although the slow deformation of solids known as creep has been known for ages, it was studied quantitatively only beginning with Andrade in 1913. The outstanding features of creep are its pronounced non-linearity and its critical dependence on chemical parameters as well as temperature. Fluids tend to become homogeneous by virtue of rapid mixing but in solids thermal as well as chemical inhomogeneities persist for long times.

There is transient as well as steady-state creep. In the study of the mantle one may confine oneself to phenomena which are of a steady-state type over all but the longest intervals of time. Quantitatively, steady creep is described by relating the stress rate, say $\dot{e}$, to the stress, σ . Below, $\dot{e}$ may be either a longitudinal or a shear strain, as the case may be, and the same holds for the stress. Observations show that the following expression gives an approximate description of creep

$$\dot{e} = \begin{cases} C_1 \, e^{-a/T} \, \sigma^{n} & (\sigma \text{ small}) \\ C_2 \, e^{-b/T} \, e^{\sigma/\sigma_0} & (\sigma \text{ large}) \end{cases} \tag{2}$$

It is apparent that the exponentials containing the temperature represent thermal activation of the atomic or molecular processes underlying creep. We also see that at large stresses the stress dependence is steeper (exponential) than at small stresses where it follows a power law. The border between large and small stresses depends on the material and on other conditions; as a rule it is of the general order of some tens to a hundred bar so that processes of mantle creep correspond more commonly to a small stress than to a large one in (2). There has been considerable variation in measurements bearing on the magnitude of the exponent, $\underline{n}$, for small stresses. Apparently there is no general rule; the behavior depends on the molecular mechanisms which are in turn dependent on chemistry, lattice structure, graininess, etc. Most laboratory values of $\underline{n}$ are between 3 and 5 (e.g., Weertman, 1962; Jones, 1965). Linear behavior, $\underline{n} = 1$, also seems to occur but since it is not the rule in the laboratory, any rash assumption of linearity of creep in the mantle is a potential source of error. We have already introduced the term ductility to express qualitatively the dependence of strain rate upon stress. We shall postpone the discussion of the mechanical properties of creep and consider next its thermal and chemical characteristics.

Experience shows that substances begin to exhibit pronounced creep when the temperature exceeds about half the melting point. They contine to become more ductile with higher temperature but do not change their behavior qualitatively until the melting point is reached. The mantle, however, is a highly inhomogeneous mixture and cannot immediately be judged by the results of laboratory experiments on chemically homogeneous substances. Since the mantle contains comparatively large fractions of lower-melting constituents we may think of the high-melting, ultrabasic materials as hard grains embedded in a more ductile matrix. Consider now Fig. 1. There is a region of closest approach to melting. In fact, there may be some substances that are at the melting point and fractionally molten in this zone, as indicated by the dashed line in Fig. 1. In any event, we can expect that there will be a <u>layer of maximum ductility</u> at some depth. It seems entirely legitimate to relate this layer to the zone of low seismic velocity discovered and so much emphasized by Gutenberg (1959). Much research on this zone has in recent years been done by Anderson (1966) and others. It appears from Anderson's data that the low-velocity zone extends from about 75 to 225 km depth, being centered at about 150 km. Of course there is no compelling reason why seismic velocity should be proportional to creep rate; the molecular mechanisms are no doubt quite different. All we are entitled to infer is, qualitatively, a change in properties of the material. Geologists have often postulated the existence of a "soft" layer at some depth, and the name asthenosphere coined by Daly is in common use. We shall employ this term here to designate a layer of greatly increased ductility whose extent, for want of more quantitative knowledge, we identify with the Gutenberg low-velocity layer.

There is one other significant index of stratification of the upper mantle which we can get from seismology. Anderson (1966) has plotted the quantity K/μ obtainable directly from seismic velocities, as function of depth (K is bulk modulus, μ shear modulus). This quantity rises first, then reaches a high level around 2-300 km depth (where $K/\mu \sim 2.0$) falls thereafter uniformly to a value of 1.81 at 800 km depth and at this point turns sharply upwards, continuing to increase at greater depth. Whatever the detailed and difficult interpretation of this behavior, depending no doubt on both temperature and pressure, it seems clear that there is a discontinuity near 800 km.

In identifying it with the phase transition from silicates to oxides we may also surmise that radioactivity as well as materials enhancing ductility have to some extent been squeezed out of the denser oxide phases to congregate higher up.

D) Chemical Factors in Creep. Among the vast variety of chemical properties that may affect creep we shall consider here only two. The first of these concerns the difference between metallic and ionic crystals. For obvious reasons most of the earlier work on creep dealt with metals. It does not seem certain a priori that ionic crystals will show the same characteristics of creep as metals but all experience indicates that there are no major differences. In more recent years extensive creep studies have been made on ceramic materials, for instance MgO. Creep in rock (marble) under high-pressure confinement was investigated among others by Heard (1963) who achieved lower creep rates than previous investigators; but his rates are still by a factor of order 10^7 above the creep rates of the mantle. On the whole, Heard's results are encouraging with regard to the use of formula (2) as a qualitative guide. But one must keep in mind that creep is found to be based on a variety of atomic mechanisms. The most important of these are, first, sliding within the relative disordered boundary layers between crystal grains; secondly the migration of dislocations across crystal grains (known as Nabarro-Herring creep). Since the material of the mantle has sojourned there for a long time, its crystals may be both quite large and also quite hard, owing to gradual loss of dislocations. It is not possible to judge crystals at depth from those found at the earth's surface since in the process of rising they usually are violently deformed. (We owe this cautionary remark to Prof. H. H. Hess.) Such deformation, by generating numerous new dislocations in the crystal grains, may radically change their mechanical behavior.

In sum, we can say rather little about the behavior of a system so far removed from laboratory conditions as the mantle in creep. We may vaguely surmise that creep is largely of the grain-boundary sliding type and this might possibly mean that creep is of the "linear" type ($\underline{n} = 1$ in equ. (2)), but this conclusion is quite uncertain. We must largely rely on empirical arguments taken from the earth in a more direct manner.

The second chemical aspect of creep to be discussed here is the capability of certain chemicals in even small concentrations to enhance the ductility vastly. These are designated as <u>fluxing</u> agents; water and the alkalis are the best known of these. So far as rocks are concerned, experiments on quartz under high-pressure confinement have had some rather spectacular results. The data obtained by Griggs and Blacic (1965) show that the addition of small quantities of water softens quartz quite radically. In the same context the softness of serpentine, a hydrated ultrabasic rock, should be remembered.

Orowan (1965) has suggested that the upper mantle including the ocean bottoms is perfused with a small concentration of water which acts as a fluxing agent and greatly increases the ductility as compared to the continents. The latter, conversely, have been desiccated in the course of time. They form therefore comparatively rigid rafts floating on the more ductile material of the upper mantle. Only in regions and during periods of intense orogeny will the continental crust become temporarily more ductile, owing to extensive magmatic intrusions which carry a great deal of water with them. On the whole, however, ocean-bottom and upper-mantle material may be assumed much more ductile than continental plates; hence the former can be expected to have suffered far more deformation than the continents. We have found this conception a most fertile working hypothesis and adopt it throughout.

Bullard, Maxwell, and Revelle (1956) advanced the idea that the near-equality of heat flow from continents and oceans may be explained by the following assumption: the large heat flow coming from radioactivity embedded in continents is balanced on the ocean bottoms by a corresponding quantity of convectively supplied heat. This view can now be put on a much more plausible basis (Elsasser, 1967). On the assumption of the Orowan hypothesis one can prove that the dynamics of the convection-generating forces is such that it tends to establish a near-equalization of surface heat flows (provided the thermal conductivities of the materials are approximately equal, which is plausible). While we cannot go into details here, it is clear that one is by no means entitled to infer unambiguously from the mean uniformity of heat flow a corresponding uniformity in the distribution of radioactive materials beneath.

4. Long-Distance Correlations

We have seen that the upper mantle forms, with respect to convection, a shallow tray whose depth, if estimated at 800 km, is only one fiftieth of the earth's circumference. There is every evidence that the surface expressions of convection involve distances very much larger than this depth. Hence the convective patterns appear to be horizontally extended, pancake-shaped. Tozer (1967) has carried out experiments which show that when a fluid is heated internally (in his case by electric currents) rather than by contact with a bottom plate, the convective pattern will be distinctly broadened, its horizontal extension becomes considerably larger than the vertical one. This model of heat sources should apply to the upper mantle. The geophysical evidence indicates well beyond this that mantle material rises rather concentratedly under submarine ridges, moves away from these horizontally, and sinks along sloping layers (the Gutenberg fault zones) marked at the surface by trenches (Fig. 2). The very long horizontal legs of such a convective pattern are no doubt in the main due to the presence underneath of the asthenosphere, a layer of greatly enhanced ductility.

Convective activity is also reflected in the worldwide gravity data from satellite observations. Runcorn (1965) has developed methods to correlate such data with the buoyancy forces inside the earth. Qualitatively the correlation is good, but it is difficult to make it highly detailed: Owing to the narrowness of the zones of upwelling and sinking, the spherical-harmonic series of the geopotential on whose lower harmonics the maps of satellite data are based, converges only very slowly.

Owing to the shallowness of the upper mantle it is profitable to study the surface topography and to look for indications of a convective pattern there. The nomenclature with regard to behavior in depth (Fig. 3) used from now on is as follows: We divide the upper mantle into tectosphere (ca. 75 km deep), asthenosphere (from 75 km to 225 km, say) and mesosphere (from there to about 800 km). These are not well-defined layers in a material sense, of course, but only symbols for dynamical entities whose depth may vary within large limits.

The general pattern of a sliding tectosphere with rising and sinking branches is shown in Fig. 2. Hess (1962) first assumed that submarine ridges do not just represent local swellings but loci of a steady upwelling with the material then moving off laterally to form ocean bottom. This model has been amply confirmed. Among the corroborative results we mention only two. The recent seismic analysis of Sykes (1967) shows directly some of the faults crossing the Midatlantic Ridge to be transform faults in the sense of Wilson (1965). This makes the interpretation of the ridge quite unambiguous; it is incompatible with any known process other than rising which then turns into lateral spreading. Again, Oliver and Isacks (1967) have given impressive seismological evidence to the effect that the slanting descent under about 45° as shown in Fig. 2, is a realistic picture. Here, where we give a theoretical model rather than a survey of the literature, we are unable to mention numerous related observations.

We now confront an interesting question: While there is some asymmetry with regard to vertical components of the motion in Fig. 2, it is not remotely as strong as the asymmetry commonly found in the convective motion of stratified fluids. To take a very familiar form of convection, the thunderstorm; we find in it concentrated vertical updrafts with velocities reaching many meters/sec; the compensating downdrafts are spread over large areas and correspondingly small, perhaps only a fraction of a mm/sec. No similar asymmetry is visible in the upper mantle. We see extensive upwellings, and the tremendous overall length of the ridges (over $7 \cdot 10^4$ km) makes it hardly necessary to look for any other places where mantle material rises,--and none are apparent. There is now the question whether in addition to the downdrafts marked at the surface by trenches there exist regions of extensive but slow subsidence. On the basis of all the information we have, our answer is that there seem to be none.

We shall strengthen this conclusion by some examples. If there is any large and regular surface below which we could expect slow subsidence, it is the Pacific. The East Pacific Rise is an intensely active ridge and the material flowing from there eastward is no doubt flowing down at the Andean Trench. To the west of the Rise there is a vast expanse of ocean but we do not see any generalized subsidence. Instead, we see the Tonga Trench, well defined, deep, and an active site of localized sinking. To take another example: The manner in which some continental blocks are surrounded by ridges leads to the inference that if material does indeed travel away from the ridges, there should be subsidence below the continental plates (Dietz, 1961; Menard, 1966). A place where this is most likely to occur, is under Africa which is surrounded on three sides by well-marked ridges. Again, subsidence could not occur under eastern Africa, where there is the well-developed Rift Valley, certainly a rising, not a sinking feature. Thus subsidence would almost certainly have to be centered on the Sahara. But Menard's map for Africa shows very clearly that the Midatlantic Ridge in the region where it passes by North Africa is shifted to the west; this section of the ridge is set off by an east-west fault at each end (roughly at 10° and 30° northern latitude). The obvious qualitative explanation of this lies in a "pull" by the Puerto Rico Trench to the west. No evidence of a downdraft under Africa is afforded by topography. Another example is the North American continent. Adherents of a continental drift model agree generally that the continent has drifted to the west and in the process has overridden the northernmost branch of the East Pacific Rise, whose extension is now under the Cordilleran Plateau. The eastern United States is therefore a natural place to look for evidence of deep and widespread subsidence under the continental plate, seeing that this region is roughly in the middle between two strips of upwelling which have been active in the fairly recent geological past. There is again no evidence.

If we speak of evidence for large-scale subsidence, we can only have in mind disturbances of the surface structure which are geologically marked. One might say that such disturbances are minimized by the relative hardness of the continental plates. It would be most surprising, however, if some such evidence could not be found on the ocean bottoms where one is so much closer to the more ductile material of the mantle. The extensive explorations of the bottom of the Atlantic by means of echo soundings leave no serious doubt that the tectonic condition of the Atlantic ocean bottom has not undergone any detectable disturbance since the Tertiary, the period when sediments began to be deposited there (see in particular the work of the Lamont Observatory, e.g., Heezen, 1962; Ewing *et al.*, 1964). Unquestionably, ocean bottom sediments would be among the most sensitive indicators of such disturbances.

The Midatlantic Ridge has been and still is at many places a very active structure. Failure to detect slow subsidence of material that has spread sideways from this ridge is highly significant; this induces us to suspend the idea that such subsidence exists until some favorable evidence, either observational or at the least theoretical, is provided. We are left then with the view that the only loci of sinking are the regions of the Gutenberg fault zones marked by trenches at the places where they intersect the ocean bottom. The crucial point of our model is therefore the sliding of the tectosphere over the more ductile asthenosphere, carrying material from the ridge to the trench.

This leads at once to the question whether there is a closer and dynamically motivated correlation between ridges and trenches. Surprisingly enough, the answer is, on the whole, no. The classical case, always cited, of the Andean Trench which runs parallel to the South Atlantic Ridge is an exception rather than the rule. Another example of a modest correlation is that of the East Pacific Rise with the Andean Trench to the east and with the Tonga Trench to the west. But here the ridge is vastly longer than these trenches and parallelism is limited. In the remaining parts of the ridge or trench system, no pairwise correlation is visible. From this we can legitimately conclude that horizontal displacements of the tectosphere or rather, of pieces of it, can take place only when there is a stretch of ridge at one end and a stretch of trench at the other. A rather spectacular example is as follows: The North Atlantic Ridge in the general neighborhood of Iceland is very active at this time; but the material issuing from there and moving sideways cannot find a trench except very far away: to the west there is the Aleutian Trench and to the east we have to go all the way to the Pacific margin of the Asian continent to find trenches. Notably enough the Aleutian Trench is far from parallel to the Atlantic Ridge.

We may conclude this series of examples with the following: The Juan de Fuca Ridge is very short, only a few hundred km long, situated in the Pacific off Vancouver Island and directed SW-NE. To understand that there can be any horizontal spreading from this ridge, one has to recall that it is about midway between two trenches, the Aleutian and the Puerto Rican. That this relationship is not just pure fancy is attested to by the San Andreas fault which begins at this ridge and runs from there over 2000 km to the south-east, through California and well into Mexico. The shear direction at the fault is compatible with a movement of the tectosphere from the region of the ridge toward the Caribbean.

Strange as these results may be, there is at least one major observational fact to support this trend of ideas. Menard (1967) has been able to trace the Clipperton fault over 10,000 km along nearly a great circle, all the way across the Pacific. Now if such a fault is indicative of sliding of pieces of the tectosphere past each other, as had been suggested by the early magnetic work (Vacquier et al., 1961), then such sliding is on a gigantic scale, 10,000 km being 90° on a great circle. Hence the idea that the mechanics of the tectosphere has some nearly world-wide aspects can be strongly supported.

The ridge system shows certain clear regularities of which the best known is the rather precise median position of the Atlantic Ridge between the continental margins. This is not the only case; the Carlsberg Ridge is median in the Indian Ocean, a ridge passes in the middle between Africa and Antarctica, etc. Menard (1967) in his penetrating study of the regularities of submarine topography says that about half of the length of the ridge system is found in such median positions. He does not consider this as the most distinctive feature of ridges; according to him this feature is their tendency to be located at a fairly constant distance from the continental margins. A number of maps of continents in polar projection given in Menard's paper put this clearly in evidence. The most conspicuous case is Africa, which is surrounded by ridges on three sides at a fairly constant distance from the edge of the continent. Antarctica is fully surrounded by ridges but the distance is not quite so regular. South America is partially surrounded by ridges but the pattern is somewhat irregular there. Another very curious example is the following: At one place the East Pacific Rise shows a rather pronounced bend which carries it in an arc around the area once occupied by the now largely defunct Darwin Rise. It is quite likely that the Darwin Rise, having once been the locus of very intense volcanic activity, has become somewhat desiccated, and hence this region of the tectosphere might in hardness be closer to typical continental than to other oceanic regions.

On the whole, the existence of strong long-distance correlations in the position of ridges is a patent fact. It is obvious that such correlations must be the expression of a system of forces; in a solid this takes the form of stresses. These stresses act over very large distances such as the width of the Atlantic and apparently even across the Pacific. But stresses must somehow propagate. We ought to remember here that since velocities in the mantle are so small, the mantle must be at any one time extremely close to equilibrium, meaning _elastic_ equilibrium. We can hardly conceive of the stresses inferred as propagating through the deep interior of the mantle. In a homogeneous solid, stresses due to a point force decrease as $\underline{1/r}^2$ with distance, $\underline{r}$, and stresses due to a line force as $\underline{1/r}$. No such impression of decrease with distance is gained from the topographic effects. Furthermore the long-distance correlations are with the margins of continents which are clearly very shallow features compared to the depth of the mantle; the influence of continental plates should be quite small below the tectosphere. We may conclude safely that stresses are transmitted horizontally, along the earth's curvature in the topmost layers, that is, in the tectosphere. The tectosphere acts as a stress guide. This is possible only because it can slide readily on the underlying asthenosphere.

The correlations of the ridges to the continental margins are of two types: In the first place the ridges tend to keep a certain average distance from these margins; in the second place when there are two roughly parallel continental margins and a ridge in the ocean basin between them, the ridge tends to appear in the median line between those two margins. There seems to be one way only in which these observations can be interpreted mechanically: the stresses in the tectosphere, whatever their nature, act in such a way as to move a ridge and a continental margin apart. Furthermore, the effect of the stresses must decrease somewhat with distance from the continents; otherwise the median position of the ridges would not be mechanically stable. Note that the preceding propositions are not a hypothesis; so long as we are careful not to specify the stress-transmission mechanism prematurely, they are no more than a translation of the observed facts into the language of mechanics.

We can draw one other significant conclusion. The ridges could not show the kind of regularity we observe unless they were mobile within the material of the upper mantle. If the ridges are upwellings, the material which rises in them must come from an appreciable depth, probably from the mesosphere, which we estimated as extending from about 225 to about 800 km depth. In the language of hydrodynamics, the ridges are dynamical, not substantial features. A dynamical pattern is one which moves across the fluid (e.g., a wave crest) keeping approximately its shape while the particles composing it change. This in turn makes it certain that the upper mantle must be rather homogeneous horizontally; otherwise dynamical features that propagate with little change of shape could not exist.

A closer look at Fig. 2 lets one recognize the difference between dynamical and substantial features. Fig. 2a may be thought of, for instance, as representing the upwelling of mantle material in the East Pacific Rise, its migrating westward and its going down at one of the trenches off southeastern Asia or Australia. This could be conceived of as a conveyor-belt operation in which the main block of mantle in the center of the moving sheets remains untouched. But Fig. 2b has a quite different topological structure: the central mass cannot remain intact in the long run. We may think of this diagram as representing schematically material which rises in the South Atlantic Ridge, then becomes tectosphere which moves to the west, carrying the South American continent with it. Room is made for this westward migration by material of the Pacific going down at the Andean Trench. The dynamic character of the pattern and the change of its constituent material particles is here quite clear.

Combining the previous arguments, we can draw a third major conclusion: The pattern of convection in the upper mantle is to a large extent controlled by the mechanical condition of the top layer, the tectosphere. We do not say that it is entirely so controlled because upwellings are found under continents also, and it would be difficult to justify this fact by any reasonably simple model of the tectosphere alone. But the fundamental role which the distribution of continents and oceans plays in the location of the ridges is clear enough. Again, this is less surprising than one might think. Convective flow is capable of a great variety of patterns, most of them not uniquely stable, and thus the influence of a boundary upon convective patterns is quite intelligible. The corrugations of the earth's surface certainly have a great influence upon the convective patterns in the lower atmosphere. In the case of the mantle a similar influence seems to be exerted by its upper, free surface, which has become corrugated and modified through the effect of chemical and thermal variations in its material and by the existence of the crust.

So far we have emphasized the ridges and have said little about the trenches. These show even more definite correlations to continental margins. In fact all present-day trenches are located around the border of the Pacific Ocean or close to it. An occasional one is shifted toward the interior of the Pacific like the Tonga Trench; others, mainly the Java Trench and the Puerto Rico Trench, are found at a moderate distance from the

Pacific. No trenches are found elsewhere in the world. Hence if tectospheric motion is from ridge to trench, the corresponding transmission of stresses must be possible over a vast scale since about half of the earth's surface has no trenches.

The distribution of trenches cannot always have been this way. One can hardly escape the conclusion that up to the Tertiary there was a trench in front of the Himalayas, and perhaps a whole trench system reaching from Spain to Szechuan, along the southern edge of the Eurasian continent. Without this assumption the orogenies along this edge would be hard to understand. Also one of the more conspicuous features of the continental drift hypothesis, the "drifting" of India from a position near the southern end of Africa to its present location could not be understood without such an assumption. The topography of the bottom of the Indian Ocean shows rather distinct traces of an original north-south fault system dating from this period, overlain by east-west faults corresponding to a more recent drift direction.

Although correlation with continental margins is clear enough, none of the facts concerning trenches have as yet found a theoretical explanation. The only generalization we can seemingly make is one already mentioned, namely that a system of drift as described will not work unless the trenches as well as the ridges are dynamical and not substantial features.

To summarize our main conclusions: <u>1</u>. <u>There is a stress system</u> (<u>details as yet unknown</u>) <u>acting horizontally along the</u>

tectosphere which tends to move ridges away from continental margins. --2. Ridges and trenches are dynamical patterns, mobile within the material of the upper mantle. --3. The actual pattern of convection through the upper mantle is largely controlled by the mechanical-thermal condition of the tectosphere.

5. Sliding of the Tectosphere

From the preceding analysis there emerges at least a qualitative picture of mantle convection. We were led to assume that radioactive heating and hence convection is confined to the layer above the Birch transition zone (phase change from silicates to oxides) at about 800 km depth. There are four legs to the convective circulation, two long, horizontal ones and two short, vertical ones. The upper horizontal leg consists mainly in the sliding of the tectosphere over the asthenosphere; one of the consequences of this sliding is the presumptive spectacle of continental drift. The other near-horizontal leg must be some sort of "return flow" in the mesosphere but the mechanism is altogether unknown. There is no reason to assume a one-to-one correspondence between a horizontal motion in the tectosphere and a transport in the opposite direction in the mesosphere. This would imply the picture of a "cell". While this concept has often been thought of, it lacks foundation in fact, and far more general patterns are readily possible.

Convection in the upper mantle differs from other convective processes in a variety of important characteristics, among them are: the extreme thermo-mechanical stratification of the upper mantle; the highly superadiabatic temperature gradient which, as the evidence shows, is not wiped out by the convection itself; non-linear creep of a solid taking the place of ordinary viscosity of a fluid.

Still, it is becoming more and more clear that upper-mantle convection is the basic mechanism that underlies most of the large-scale tectonic phenomena observed on the earth. The velocities of order one to several cm/year, which observations lead us to ascribe to convectively induced displacements, exceed by orders of magnitude those that would follow from the older concepts of a steadily contracting or expanding earth. In a detailed paper, Orowan (1967) has recently shown that both contraction and expansion could be ignored as causes of the observed amounts of tectonic deformation even in the absence of convection. Therefore, convection appears the more as the essential basic mechanism of tectonics.

We shall here concentrate on the horizontal displacements of the tectosphere as that branch of the convective upper-mantle circulation that is most readily accessible to some degree of theoretical analysis. Borrowing a term from oceanography and meteorology, the writer (1966) has used the term "advection" to describe such essentially horizontal motions. Ringwood and Green (1966) have adopted this suggestion but use the term in an extremely broad sense, almost synonymous with convection. We would suggest, however, that the term be restricted to fully or almost horizontal motions so as to make its meaning the same in the various branches of geophysics.

In order to gain any insight into the mechanical conditions of upper-mantle convection, it is almost inevitable that one begin with a model of a viscous fluid, that is, a substance with linear viscosity and incapable of purely elastic deformation. It must remain open how far this is a justifiable approximation. In terms of a viscous fluid, then, we represent the asthenosphere by a sharp decrease with depth of the viscosity, η. A sketch is shown in Fig. 3 where log η is plotted against depth in a more or less arbitrary fashion. We assume here a rapid decrease of η from the surface to a minimum in the asthenosphere, by about a factor 10^2-10^3, and then a much slower rise on going down into the mesosphere. The last-named, much slower trend seems indispensable if one is to account for a "return flow" in the mesosphere. The latter is subjected to the severe constraint of having no free boundaries; but since return flow must necessarily occur if the tectosphere moves, there must be some mechanical condition that facilitates flow. The only one that comes to mind for the mesosphere, in the absence of any evidence for a "soft" layer farther down, is an increased ductility over an appreciable interval of depth.

To make numerical estimates about the sliding of the tectosphere over the asthenosphere, we require some further drastic simplifications. We represent the tectosphere by a <u>solid</u> plate of thickness h_1 and elastic modulus E. We represent the asthenosphere by a fluid layer of depth h_2 and viscosity η. In steady translational motion of the tectosphere the velocity distribution will be as shown in Fig. 4. We now assume that the shear stress exerted at the bottom of the plate is balanced by the total horizontal stress exerted at the end of the plate. The fluid is assumed at rest at the bottom of the asthenosphere. The velocity, v_x of stationary sliding is readily found to be

$$v_x = \frac{\sigma_x}{\eta} \cdot \frac{h_1 h_2}{L} \tag{3}$$

where L is the length of the sliding plate. (Calculations refer to unit thickness perpendicular to the plane of Fig. 4.) Now take $\eta = 10^{21}$, smaller than the often accepted value for the upper layers of the mantle but perhaps reasonable for the asthenosphere. Letting for simplicity $h_1 = h_2 = 100$ km and $L = 1000$ km, and assuming $v_x = 1$ cm/year we obtain $\sigma_x = 30$ bar. This shows that the concept of a sliding tectosphere makes good mechanical sense, at least for a crude model.

Next, consider transient conditions. Under lateral transient stress the plate becomes locally compressed or extended. We assume these effects first to be purely elastic; then, if u is the displacement of a point on the plate, the local horizontal stress is $\sigma_x = E\, \partial u/\partial x$, and the horizontal force per unit slice of the plate is $h_1 \partial \sigma_x/\partial x$. On the other hand, the shear stress at the bottom is $\eta v_x/h_2 = (\eta/h_2)\partial u/\partial t$. (Here, all motion is assumed slow so that $d/dt = \partial/\partial t$.) Requiring again balance of the horizontal forces with the shear forces on the plate we obtain finally

$$\frac{\partial u}{\partial t} = \kappa \frac{\partial^2 u}{\partial x^2} \quad , \qquad \kappa = h_1 h_2 E/\eta \qquad \text{(4a, b)}$$

Equation (4) has the form of a heat-conduction or diffusion equation; its integration is standard. In particular, the mean distance x by which a disturbance propagates in time t is given approximately by

$$x \sim \sqrt{4\kappa t} \qquad (5)$$

Letting $E = 1.6 \cdot 10^6$ bar (Bullen, 1963), the other quantities being as in (3), we find $\kappa = 1.6 \cdot 10^5$; and for $t = 10^4$ years, (5) gives $x = 1500$ km.

While these values are not radically different from those frequently quoted from the analysis of the Fennoscandian uplift, it is of importance here to remark that the definition (4b) of κ has no real physical meaning. Orowan (1958) has pointed out that a plate of rock subjected to lateral forces will always react plastically, not elastically, provided the thickness of the plate is appreciably in excess of 10 meters. This follows from standard formulas of the mechanics of solids and may therefore be safely accepted. The tectosphere will thicken on compression and be thinned under tension. Isostatic mechanisms will then come into play to restore equilibrium. If the deviations from equilibrium are small, one may tentatively assume that the restoring forces are linear in these deviations and (4a) will again hold, but with a different physical significance and numerical value of the coefficient, $\underline{\kappa}$. From the orders of magnitude observed in the postglacial rebound, it is rather safe, however, to assume that $\underline{\kappa} = 10^4 - 10^5$ in order of magnitude, and hence horizontal stress propagation is rather fast on a geological time scale. For our purposes we need not specify the detailed mechanism of this propagation (see also Beloussov, 1962). Of course the stress will diminish somewhat as it propagates. Still, this picture of horizontal propagation, where penetration into depth is buffered by the asthenosphere, is quite different from the stress pattern that a load would produce in a homogeneous spherical shell.

Next, let us compare sliding of the tectosphere with what would happen if material is moved <u>inside</u> the asthenosphere. If for instance the tectosphere buckles down, there should be a lateral outflow in the asthenosphere. The following simplified model will suffice (Landau and Lifshitz, 1959). The space between two parallel circular plates of radius $\underline{R}$ and mutual distance $\underline{h} \ll \underline{R}$ is filled with a viscous fluid. The plates are compressed with a force $\pi R^2 \sigma$ where σ is the normal stress. The mean velocity (averaged over h) at the rim is

$$v = \frac{\sigma}{3\eta} \cdot \frac{h^2}{R} \tag{5}$$

Dimensionally, (5) resembles (3) but the difference is readily apparent. We get large velocities in (5) only when the normal stress applies over large areas; if stresses act only in limited regions the sliding motion is strongly preferred (provided of course the sliding plate is not stopped at the far end). On the other hand we see from (5) that large h increases the velocity of internal motions; these motions then will tend to be more closely isometric, that is the horizontal dimension should be comparable to the vertical ones, both presumably of order of a few hundred km. Whether such internal localized circulations exist is not known at present. Some other examples of very slow viscous flow are calculated in Jaeger (1962).

All arguments just given are based on a linear law of viscosity. If we have a non-linear relationship between strain rate and stress (such as (2) with $n > 1$) the creep patterns will as a rule change in such a way that large strains become concentrated and enhanced and small strains tend to be reduced. Thus while a viscous fluid flows through a tube with the well-known parabolic velocity profile, a plastic substance has its shear concentrated at the boundary, i.e., it moves like a plug. Orowan (1965) has suggested that the ridges correspond to large rising blocks with the shears concentrated at the edges;--however, seismic data point rather to a strong concentration of the shear in a narrow central band of the ridge. Since the material may undergo phase transformations as it rises, the elevation of the ridges would then be attributed to this less dense material having flowed sideways from the central current.

Nonlinearity of the type (2) is complicated by time-delays in solids. There is strain hardening and strain softening, both well known in the laboratory. Earthquakes are due to non-linear strain concentration together with strain softening of an as yet obscure type. It is remarkable that there are no earthquakes that would be indicative of a horizontal sliding of the tectosphere. If the creep law is highly non-linear the layer of sliding will tend to be very concentrated as shown by the dashed line in Fig. 4. The fact that no corresponding quakes are observed may indicate that the non-linearity is not very pronounced,--but definite conclusions seem premature.

Regarding earthquakes: it is no doubt significant that practically all the deep-focus quakes (below about 200 km) are in regions where one would suspect material to sink. Perhaps the sinking material has first suffered loss of water or other fluxing agents fairly close to the surface; farther down it undergoes phase transformations to denser forms which makes it sink still farther. This material is likely to be far less ductile and therefore probably more subject to shear disruption than average mantle material at a similar depth. Earthquakes are indicators of motion but the reverse is not true: their absence does not prove absence of motions.

Analysis of observations shows directly (Morgan, 1967) that large sections of the tectosphere slide or slide-and-rotate. This can be achieved by three kinds of stresses:

(1a) local horizontal compression

(1b) local horizontal tension

(2) shear drag from underneath.

Clearly, (2) can coexist with either (1a) or (1b). Applicability of (2) would imply that the motion beneath the tectosphere is faster than the motion of the tectosphere itself. The discussion following equation (5) indicates that such flow underneath would be of limited horizontal extent, say a few hundred km across. So far there is no clear evidence for the existence of such undercurrents.

Compressive stresses will occur at the flanks of ridges and tensile stresses adjacent to trenches; it is only when the two act simultaneously on a piece of the tectosphere that motion of the latter can occur. (The singular exception would be a circular piece of tectosphere which could rotate about its center without creating gaps or overlaps.) Little can be said about the tensile stresses since little is known. Material which by various processes has been made denser and slides down under about 45° along the Gutenberg fault zones, can either be "pushed" down or it can sink down on its own by having acquired a sufficiently greater density than surrounding mantle material. Several arguments suggest that the second of these alternatives is applicable. The motion along the 45° zones then leads to a tensile "pull" in the adjacent horizontal part of the tectosphere by simple mechanical means which we shall not analyze here.

The lateral compression due to ridges offers some more interesting features which we now discuss. With a height of the ridge, say, 2 km above sea-bottom, the excess vertical pressure is around 600 bar. Now a piece of solid that is subjected to uniaxial compression has zero stress in a perpendicular plane only if laterally unconfined; if confined it has a lateral compressive stress. If stress relaxation occurs this lateral stress grows asymptotically toward the full vertical stress (hydrostatic pressure). Very little is known either experimentally or theoretically about stress relaxation. Whether it occurs can sometimes depend on boundary conditions (Fig. 5). In Fig. 5b horizontal stresses will continue almost indefinitely as the material spreads to the sides. On the other hand, it is intuitively evident that while stress relaxation in Fig. 5a must occur, it cannot depend on the particular geometry. One expects it to be an intrinsic property of the material. There is one quantity having the dimensions of a time which can be formed from the <u>macroscopic</u> parameters of the material, namely, $\underline{t_r} = \underline{\eta/E}$, where we now interpret η as the (linearized) viscosity for slow creep, assuming that such a quantity can be meaningfully defined in order of magnitude. Note that the often used value, $\eta = 10^{23}$, derived from the postglacial uplift, assumes a priori a viscous fluid, that is, it assumes that stress relaxation has already occurred. With this value of η we find $\underline{t_r} = 2000$ years. Thus, unless this value of η should be completely meaningless, stress relaxation is much faster than the postglacial uplift. It may be safe to assume that η is somewhat smaller on ridges than it is under Scandinavia, and the stress under ridges might thus not be far from hydrostatic at all times. Thus we get appreciable forces for a lateral push on the tectosphere.

A remark on the nature of stress relaxation: This does not mean the disappearance of the convection-generating forces. It means the conversion of a local stress tensor into an isotropic hydrostatic pressure. A tendency to relaxation always exists since relaxation diminishes the stress energy. There remain, after relaxation, the circulation-generating forces of hydrodynamics: such forces appear whenever the surfaces of constant pressure do not coincide with the surfaces of constant density. There is no implication in this that the viscous friction must be linear.

On the earth, isostatic equilibrium, which means balance of vertical forces only, is often rather closely fulfilled. The uncompensated horizontal forces should lead to slow horizontal expansion or contraction, as the case may be. This was first emphasized by Benioff (1949) for the continental plates but very clear evidence is lacking for these. Orowan (1958) suggests that the breakup of Northern Canada into an assembly of islands may be attributed to a "Benioff spreading stress" of this kind. If continental plates are less ductile than ocean-bottom material, the spreading stresses of the latter, issuing from the ridges, should preponderate. In fact, the well known double maximum of the terrestrial height-distribution curve (e.g., Scheidegger, 1963) with a strong concentration of heights near sea level and another strong concentration at the level of the abyssal deeps (4-5 km) indicates decisively that there must be a lateral force holding the continents together and keeping them "thick", as it were.

This would conclude our argument except for the need of avoiding one likely misconception. Sliding of the tectosphere as understood here is not identical with "sea-floor spreading" as deduced from magnetic observations (Vine and Matthews, 1963; Vine, 1966). At large distances from the center of a ridge the basaltic crustal layer, which may be assumed to contain the magnetization, is no doubt carried along passively by the moving tectosphere, and to this extent the two are equivalent. But if the model of material rising under ridges and then spreading out to form new tectosphere is correct, there is a large discrepancy: A zone of stagnation should appear in the middle of the ridge where the horizontal velocity from being zero in the middle increases slowly as one goes outward. One may estimate that the width of this zone should be comparable to the depth of the convective layer, thus at least 200-300 km. But the observed zone of stagnation for the magnetically observed spreading is only a few km wide. To explain the difference, we must take note of the structure of the oceanic crust. Echo soundings show a thin upper layer of sediments, below it a layer of about 1 km thickness, presumably basalt, and below this a layer roughly 5 km thick resting on top of the mantle. According to Hess (1955, 1962) this lower crustal layer is largely made up of serpentine, that is, hydrated olivine. Serpentine is a notoriously highly ductile substance and retains high ductility under pressure (Raleigh and Paterson, 1965). So we are tempted to attribute the extreme narrowness of the zone of stagnation and the gradual transition from there into motion synchronized with a sliding tectosphere, to the easy deformability of serpentine. The magnetically observed motion would be that of the crust which is not necessarily identical with that of the mantle. In our earlier analysis we have therefore relied on features of macrotectonics rather than on specific magnetic data.

__Acknowledgment:__ The research leading up to this paper was supported by a grant from the National Science Foundation and by a grant from the National Aeronautics and Space Administration.

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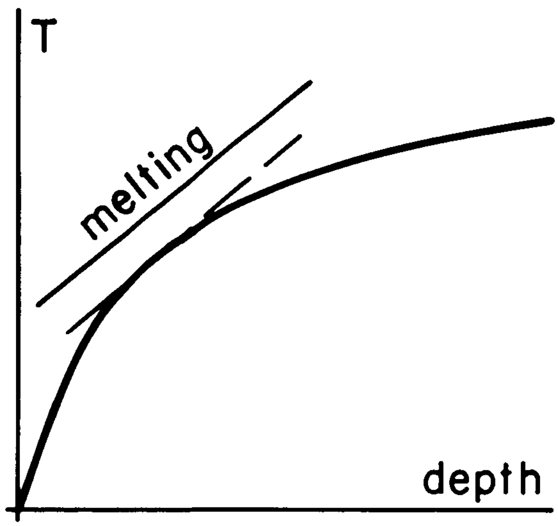


Fig. 1

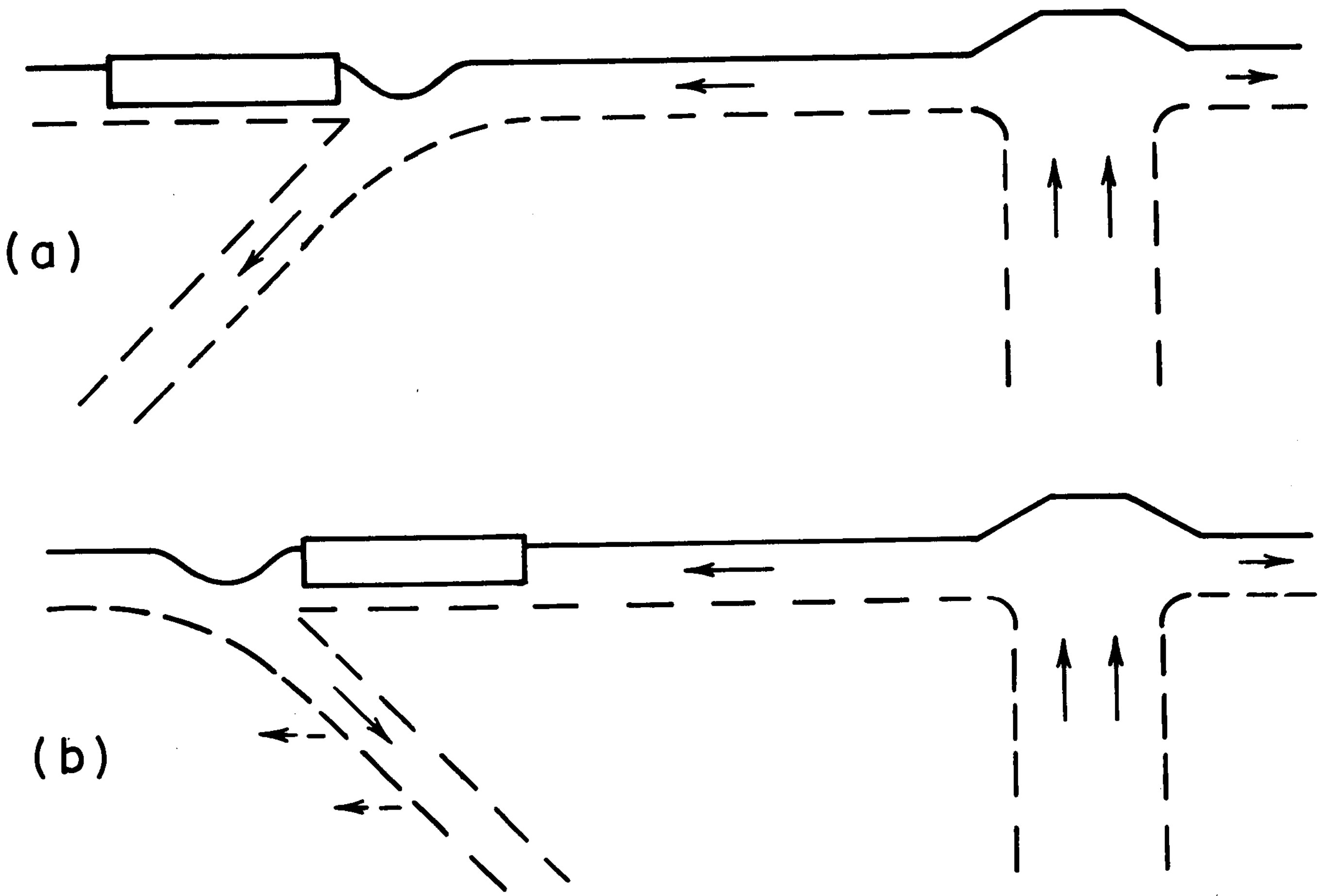
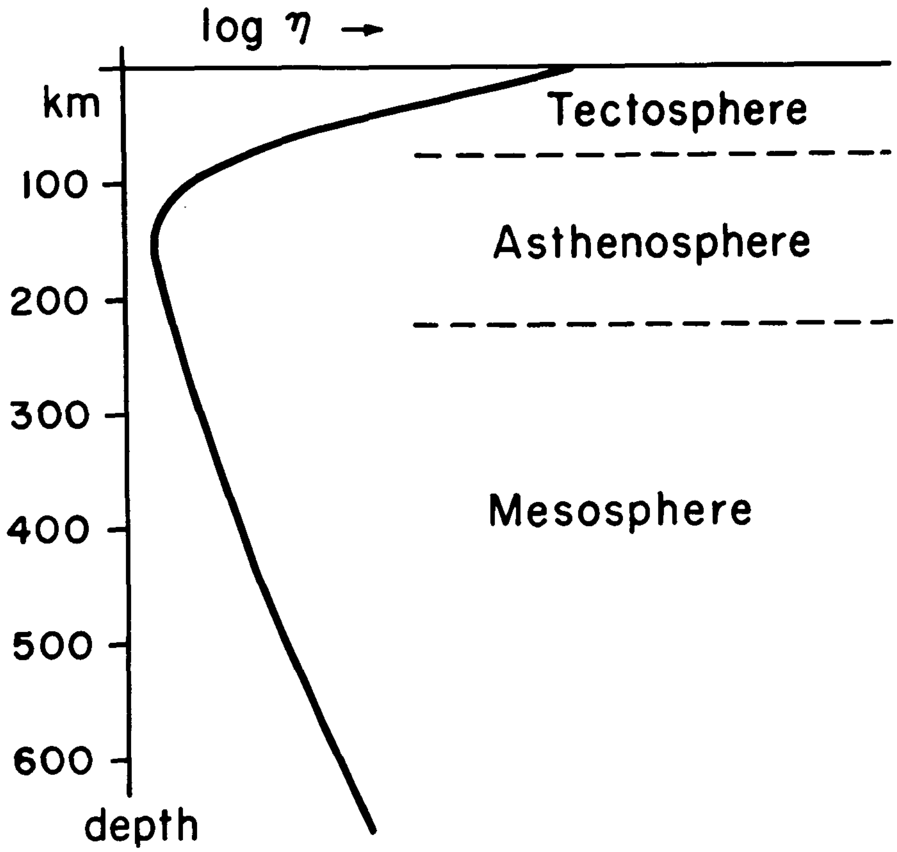


Fig. 2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km
100
200
300
400
500
600
depth
Tectosphere
Asthenosphere
Mesosphere

Fig. 3

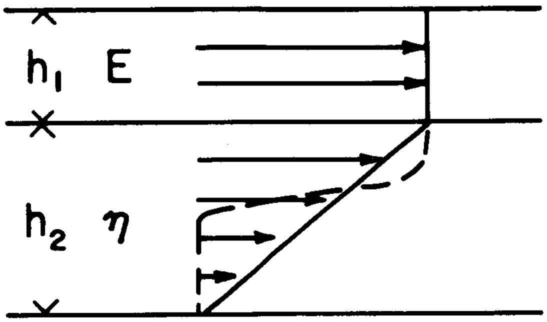

Fig. 4

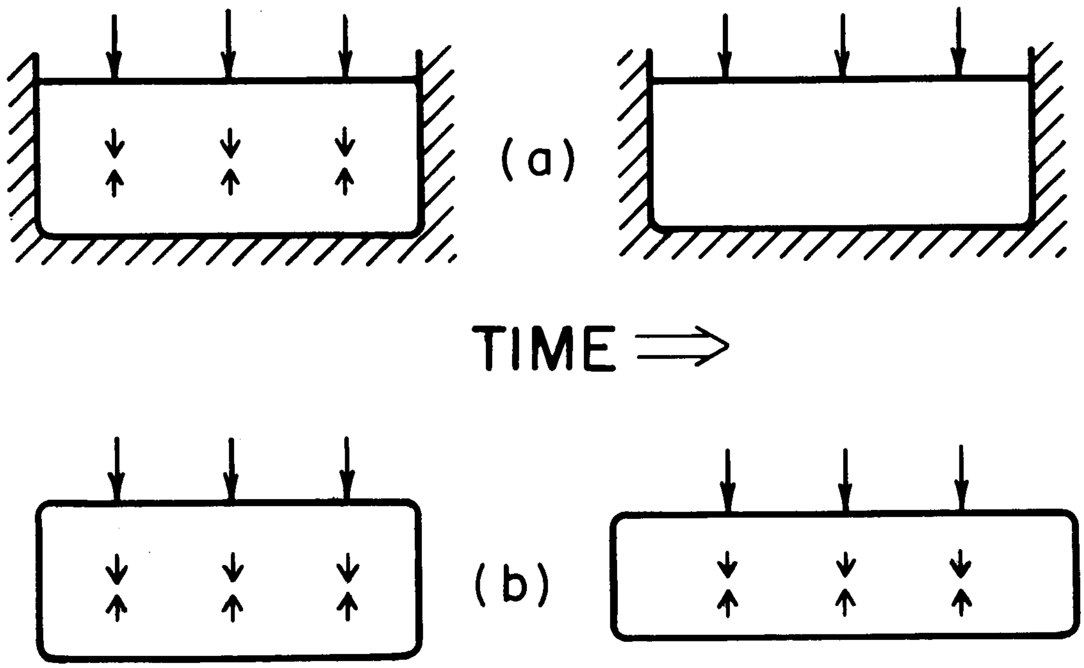

Fig. 5

Previous Publications under Grant NsG-556

1.) B. Durney: Stresses induced in a purely elastic Earth Model under various tectonic Loads, Geophys. J. Roy. Astron. Soc., 10, 163-173, 1965

2.) B. Durney: Equilibrium Configuration between Density and Surface Irregularities in a Purely elastic Earth Model, J. Geophys. Res., 71, 3029-3032, 1966

3.) W. J. Morgan: Gravity Anomalies and Convection Currents, 1. A Sphere and Cylinder sinking beneath the Surface of a viscous Fluid, J. Geophys. Res., 70, 6175-6187, 1965

4.) W. J. Morgan: Gravity Anomalies and Convection Currents, 2. The Puerto Rico Trench and the Mid-Atlantic Rise, J. Geophys. Res., 70, 6189-6204, 1965